THE MOTIVATING EFFECTS OF EXTEROCEPTIVE STIMULATION

D. O. HEBB

McGill University

MY instructions from the man who cracked the whip—in the role of Chairman of this symposium—were to center my talk about the effects of perceptual deprivation on human motivation and the study of brainwashing made at McGill. It's obvious, no one can *really* deal with my topic in a 20-minute paper and also go on and talk about broad implications. So I'll make no pretence at covering the field. But if I see a chance to make a broad remark, of course, I will.

THE INFANT'S ENVIRONMENT

What we are considering here is the relation of the mammal to his sensory environment. To put this in perspective I shall start with perceptual deficit in infancy, as far as it bears on motivational and emotional problems. As you know, it is now clear that such a deficit also produces intellectual defects at maturity (1, 7, 12), but this I am not directly concerned with, except as it bears incidentally on social intelligence. Also, I am going to restrict this further to the dog, an animal that shows *some* of the complexities of temperament found in the higher species—more so than the rat, certainly. The work I refer to is mainly that of Thompson, Melzack, Heron, and Mahut (5, 14, 16–18, 20–23). These studies of temperamental variables have shown, like those on intelligence, that the normal development of behavior depends on a normal perceptual environment. The animal reared in isolation is a permanent screwball at maturity: motivationally, socially, intellectually abnormal.

These rearing experiments, of course, relate to the early observations of Spitz (19) concerning "hospitalism" and social deprivation of the hospitalized infant, and to the later observations of Bowlby (4) and others, also on hospitalized children. It is important to mention also such reports of single human subjects, reared in pretty extreme isolation, as those of Davis (6), Hill and Robinson (11), and Mason (15). But in these human cases, of course, there are a number of uncontrolled variables, making interpretation difficult indeed.

The animal experiments not only have the advantage of experimental control; they also make it possible to observe long-term effects, because the dog grows up in a reasonable period of time. It is quite practical to rear dogs in different ways and test not only at adolescence, so to speak, but also in young middle age. There is a lot of talk in the literature about how to rear the human animal so as to produce the kind of motivations ("personality") you want, at maturity; but the child psychologist sometimes forgets to what a great extent these statements depend on some theoretical formulation or other, and there aren't any theories that we can really trust that much.

There is no evidence whatsoever that shows factually, reliably, the effects of a particular kind of rearing, for the first six years of life, upon personality and so forth when the human subject has grown up. It's a long step from dog to man, of course, and we can't put much trust in this kind of inference either. The ideal subjects for experiment would be the anthropoid apes, but these take almost as long to grow up as man does, so this again is not really practical. Perhaps Harry Harlow will manage the trick in his rearing experiments with rhesus monkeys, but in the end we are going to have to depend on a better development of basic theory, combined with the data of animal experiments.

Anyway, let me summarize the dog-rearing experiments, as far as they bear on motivational questions. One point, first, is most important in the interpretation of the results. The dogs reared in isolation showed none of the physical debility and susceptibility to infection that Spitz reported. They developed exceptionally well: in fact, being nice and stupid, they made excellent show dogs; and William Ponman, who was in charge of the colony, filled a small display cabinet in the laboratory with first-prize ribbons won with our dogs, year after year, in dog shows. This physical vigor should be kept in mind in considering the motivational deviations.

The restricted dogs were markedly atypical in activity measures, exploratory behavior and the search for variety, sharing of food in a single food-dish, social responsiveness to another dog, response to either threatening *or* friendly persons, and response to pain stimulation.

They were also low in formal problem-solving ability, of course, and this brings me to the question of their "social intelligence." Their behavior suggested repeatedly that the motivational differences, or what seemed motivational differences, were partly due to not perceiving the situation in the same way as the normal dogs and not having acquired normal ways of dealing with others. "Social intelligence" used to be in the literature, along with the abstract and mechanical varieties of intelligence, and one of the things suggested by these experiments is that it ought still to be in the literature.

How do social skills develop? It's no longer possible just to take for granted that your heredity will do it. You don't grow up to be socially perceptive, sensitive to another's attitudes, able to be friendly without overdoing it, able to conceal your own attitudes as need be, and so forth—you don't develop all the social skills that one must have to live with others simply as a matter of heredity and growth. We know, with reasonable certainty, that this aspect of intellectual function, like others, must depend on experience also. But what kind of experience, when, at what age?

This symposium of course is concerned with motivation, not cognitive processes as such; but the point is that motivation is also a function of perceptions and skills. We want to bring up children with "democratic" values; if in fact this is to be so, they must know how to put them satisfyingly into effect. It is hardly realistic for the social scientist to concentrate only on establishing desirable motivations—how long can one expect them to survive, not to extinguish, if the corresponding social skills are missing?

THE ENVIRONMENT AT MATURITY

Now we come closer home, with studies directly concerned with motivation in the adult. The infant-environment work shows that the adult is a product both of his heredity and physical environment (as necessary for growth) and of his perceptual experience during the growth period. Once development is complete, does the organism then become less dependent psychologically on sensory stimulation? When a man's or a woman's character is formed, his or her motivations and personality pattern established, is character or personality an entity that exists so to speak in its own right, no matter where or in what circumstances (assuming physical health and reasonable bodily welfare)?

In the Korean war the Chinese Communists gave us a shocking answer: in the form of brainwashing. The answer is No. Without physical pain, without drugs, the personality can be badly deformed simply by modifying the perceptual environment. It becomes evident that the adult is still a function of his sensory environment in a very general sense, as the child is.

I am not going to ask you to listen again to all the details of the experiments that have been done and are still being done in this country and Canada (though the Canadian experiments are over) to investigate the problem. The work of Heron, Bexton, Scott, and Doane (2, 9, 10) began when the Defence Research Board of Canada asked us in 1952 to find out what we could about the basic phenomena, with the hope that some possibilities for protection against brainwashing might turn up. Now brainwashing, as you know, takes different forms and can involve lack of sleep, fatigue, and hunger; and it makes a lot of use of having the subject write out "confessions" (or whatever you want to call them). Only one aspect was picked out for study: isolation from the environment. The isolation was drastic, but far from complete. Visual perception was completely prevented; auditory perception was cut down, perhaps, to about a quarter of normal; tactual perceptions, to perhaps a tenth of normal (but don't ask how this quantification is done!).

The result, again in brief, was an acute disturbance of the normal personality. (The effect observed by Lilly, 13, was apparently even greater than in the McGill studies.) There were great swings of motivation, which alternated between periods of apathy and an intense desire to get back to a normal environment. Any variation of sensory input was welcomed, but with this there was a lack of energy for problem solving; and, after leaving isolation, the subject found it difficult or impossible to get back to his normal work habits for about 24 hours. In addition, there were some handsome visual hallucinations, disturbances of perception of the self, impairment of intelligence test performance, changes in the EEG, and marked visual disturbances on first emerging from isolation.

With the possible exception of the effects of propaganda, the changes were reversible, disappearing in a day or so. For the problem of brainwashing we learned something of value, which should be as widely known as possible, since we do not know who will fall into Communist hands in the future and be subjected to this—appalling, indecent, choose your own adjective—this atrocious procedure; and knowing something about it may mitigate its effects.

First, the occurrence of hallucinations can itself be terrifying to the naive subject and help to break down his resistance. If he knows that hallucinations are "normal" in these circumstances and that the effects are quite reversible, he has at least a little protection.

Second, the subject should know that his critical thinking will be impaired and that he is especially vulnerable to propaganda after being in radical isolation. Knowing this may help to resist propaganda, if the subject can keep reminding himself to make an extra critical effort. Heron's group did not try to investigate means of resistance, but did demonstrate the vulnerability to ridiculous propaganda. The subject was given talks on ghosts, poltergeists, ESP, and so forth, the experimenters deliberately propagandizing and having no truck with scientific detachment or such like. He was told that scientists are biased against psychic phenomena, that there is plenty of evidence to show the existence of ghosts, and so on—the statements being made as persuasive as possible. Control subjects were paid to listen to the same stuff and were influenced by it, but the experimental subjects significantly more. Part of this was no doubt due to the subject's eagerness to *listen,* to almost anything; again, if the prisoner being brainwashed knows in advance that he will have this weakness, he may be able to some extent to guard against it.

The effects of the propaganda were the only ones that showed signs of lasting beyond the experimental period. The groups tested two weeks later were too small to establish the point definitely, in a statistical sense, but the tentative conclusion was reinforced by incidental reports from the subjects. A number of the experimental subjects, unlike the controls, went to the library to borrow books on psychical (*not* psychological) research, mind reading, and so forth; there were spontaneous reports of being afraid of ghosts, late at night, for the first time in the subject's experience; and reports of trying to use ESP in card-playing.

It is hardly necessary to say that the experiment, taken as a whole, was very unsettling to us. Our subjects were of course free to walk out on the experiment at any time they chose (as soon as they felt they could give up the $20 a day pay!), but it would be very different for a man in fear of his life, with no choice in the matter and no termination in sight. It is one thing to hear that the Chinese are brainwashing their prisoners on the other side of the world; it is another to find, in your own laboratory, that merely taking away the usual sights, sounds, and bodily contacts from a healthy university student for a few days can shake him, right down to the base: can disturb his personal identity, so that he is aware of two bodies (one hallucinatory) and can not say which is his own, or perceives his personal self as a vague and ill-defined something *separate from his body,* looking down at where it is lying on the bed; and can disturb his capacity for critical judgment, making him eager to listen to and believe any sort of preposterous nonsense.

Fundamentally, this raises the whole question of the relation of man to his sensory environment, and it bears particularly on research in personality and social processes. There are other aspects of the problem, but for those I refer you to the chapter by Thompson and myself (8) in Lindzey's *Handbook of Social Psychology.* What I want to do now is to bring the discussion closer to everyday living.

The effects I have been talking about can occur in varying degree. Lilly (13) has shown that making isolation more drastic produces motivational and emotional disturbance much more quickly; but he has also shown that the lonely man, in not too abnormal an environment otherwise, may suffer gross disturbances when the social deprivation is long continued. Lilly has reviewed some of the published reports of solitary sailors, Arctic groups in the long polar night, and shipwreck survivors in a small boat. These people, the ones who survive, develop one or other of the symptoms of the mentally ill—and among the many who do not survive, of course, there are those who develop outright psychosis. Mystical feelings of oneness with the universe develop; fears of being insane, or of seeming to be insane, are common. In larger groups, socially closer to normal situations, Boag (3) has described a motivational process that parallels—but on a more extended scale—a puzzling aspect of results found in the McGill experiments. The experimental subjects were always eager to be tested, as a break in the monotony, and then paradoxically lacked the energy to work on problems even of moderate difficulty. Similarly, Boag reports that men in Arctic stations would make extensive plans for spare-time activities, exactly the sort of thing that would protect them against their lack of stimulation, only to slump later into a kind of apathy. There was:

> . . . apathy, lack of interest in surroundings, motor retardation, greatly increased hours of sleep, lack of attention to personal appearance and tidiness of quarters, and disinclination to undertake extra work or odd jobs, *in spite of complaints of not having enough to do* [italics added]. An occasional man will spend a whole winter in his quarters without leaving them to visit neighbors half an hour's walk away . . . (3, p. 445).

Clearly, man's motivation is a function of his exteroceptive stimulation. I don't need to labor this further, but instead draw your attention to another set of phenomena that don't get into the books. These concern the relation of our own work habits to an accustomed environment, and the ease with which motivation is broken down. How many of you, when you get back from these meetings, will be able to go on with your work on the first day, as if you had never been away? How many of you go on vacation with plans for work that do not get carried out—that is, not even to the extent that you carry out plans when you are in your normal work environment? How many of you can write only in a particular setting, after elaborate preparation, with everything just so?

It was Karl Menninger in a personal communication who drew my attention to the similarity of the deficient-environment effects and the phenomenon of not being able to get back to work after being away—I believe it is quite common, but at any rate Menninger and I suffer from it.

Being often my own best subject when it comes to the study of abnormal behavior, I have now finally to report some peculiar behavior of my own bearing on this question of the relation of motivation to the accustomed environment. I do so with the idea that others may have observed similar phenomena and may perhaps have better ideas as to how they are to be understood. The peculiarity, really, is only in certain time relations. I have repeatedly found, when I have been a visiting lecturer or away from my usual habitat in some other similar role, that my ability to write, or do other work that I jokingly call creative, runs down over a two-week period or thereabouts. It can be restored by a short period in my own laboratory setting, and then again runs down. This is presumably a function of homesickness, of what Hunt, I believe, called cryptic nostalgia—an acute condition, at times, from which I as well as many other persons suffer, though it is not mentioned among adults. (Rather vulgar, one gathers.) I can understand the nostalgia, in the sense that it is part of the whole picture of man's motivation as dependent on an accustomed environment, but I have more trouble understanding why the clock, so to speak, takes so long to run down. There are some problems here for research that have the happy combination of both theoretical and practical interest. I wish someone would tackle them.

REFERENCES

1. BEACH, F. A., & JAYNES, J. Effects of early experience upon the behavior of animals. *Psychol. Bull.*, 1954, **51**, 239–263.
2. BEXTON, W. H., HERON, W., & SCOTT, T. H. Effects of decreased variation in the sensory environment. *Canad. J. Psychol.*, 1954, **8**, 70–76.
3. BOAG, T. J. The white man in the Arctic: A preliminary study of the problems of adjustment. *Amer. J. Psychiat.*, 1952, **109**, 444–449.
4. BOWLBY, J. Maternal care and mental health. *World Health Organization Monogr.*, 1951, No. 2.
5. CLARKE, R. S., HERON, W., FETHERSTONHAUGH, M. L., FORGAYS, D. G., & HEBB, D. O. Individual differences in dogs: Preliminary report on the effects of early experience. *Canad. J. Psychol.*, 1951, **5**, 150–156.
6. DAVIS, K. Extreme social isolation of a child. *Amer. J. Sociol.*, 1940, **45**, 554–565.
7. FORGAYS, D. G., & FORGAYS, JANET. The nature of the effect of free-environmental experience in the rat. *J. comp. physiol. Psychol.*, 1952, **45**, 322–328.
8. HEBB, D. O., & THOMPSON, W. R. The social significance of animal studies. In G. Lindzey (Ed.), *Handbook of social psychology.* Cambridge, Mass.: Addison-Wesley, 1954.
9. HERON, W. The pathology of boredom. *Sci. Amer.*, 1957, **196**, No. 1, 52–56.
10. HERON, W., DOANE, B. K., & SCOTT, T. H. Visual disturbances after prolonged perceptual isolation. *Canad. J. Psychol.*, 1956, **10**, 13–18.

11. HILL, J. C., & ROBINSON, B. A case of retarded mental development associated with restricted movement in infancy. *Brit. J. med. Psychol.,* 1929, **9**, 268–277.
12. HYMOVITCH, B. The effects of experiential variations on problem-solving in the rat. *J. comp. physiol. Psychol.,* 1952, **45**, 313–321.
13. LILLY, J. C. Mental effects of reduction of ordinary levels of physical stimuli on intact, healthy persons. *Psychiat. res. Reports,* 1956, No. 5.
14. MAHUT, HELEN. Breed differences in the dog's emotional behavior. *Canad. J. Psychol.,* in press.
15. MASON, MARIE K. Learning to speak after six and one-half years of silence. *J. speech Disorders,* 1942, **7**, 295–304.
16. MELZACK, R. The genesis of emotional behavior: An experimental study of the dog. *J. comp. physiol. Psychol.,* 1954, **47**, 166–168.
17. MELZACK, R., & SCOTT, T. H. The effects of early experience on the response to pain. *J. comp. physiol. Psychol.,* 1957, **50**, 155–161.
18. MELZACK, R., & THOMPSON, W. R. Effects of early experience on social behavior. *Canad. J. Psychol.,* 1956, **10**, 82–90.
19. SPITZ, R. A. Hospitalism: A follow-up report. *Psychoanal. Study of the Child,* 1946, **2**, 113–117.
20. THOMPSON, W. R., & HERON, W. The effects of restricting early experience on the problem-solving capacity of dogs. *Canad. J. Psychol.,* 1954, **8**, 17–31.
21. THOMPSON, W. R., & HERON, W. The effects of early restriction on activity in dogs. *J. comp. physiol. Psychol.,* 1954, **47**, 77–82.
22. THOMPSON, W. R., & MELZACK, R. Early environment. *Sci. Amer.,* 1956, **194**, No. 1, 38–42.
23. THOMPSON, W. R., MELZACK, R., & SCOTT, T. H. "Whirling behavior" in dogs as related to early experience. *Science,* 1956, **123**, 939.